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XIX. *Experiments and Observations on Charcoal: By Joseph Priestley, LL. D. F. R. S.*

Read April 5,  
1770. **A**MONG the original experiments, published in the History of Electricity, was an account of the conducting power of charcoal. This substance had been considered by electricians, in no other light than that of mere perfectly baked wood, which is known to be no conductor of electricity. I have even heard of attempts being made to excite it; and though those attempts were ineffectual, the failure of success was attributed to other causes than that of charcoal being no electric substance; so fixed was the persuasion, that water and metals were the only conducting substances in nature. The consideration of the chemical properties of charcoal, which are, in many respects, remarkably different from those of the wood from which it is made, might have led them to suspect, that since, after its being reduced to a coal, it was become quite another thing from what it was before, it might possibly differ from wood in



this property; but this consideration had not been sufficiently attended to.

In the account of my former experiments on charcoal, I observed, that there were very great differences in the conducting power of charcoal, and particularly of wood charcoal, though I could not determine on what circumstances in the preparation, &c. those differences depended. I thererore expressed a wish, that some person, who had conveniences for making chemical experiments, would prosecute the inquiry, as one that promised, not only to ascertain the cause of the conducting power of charcoal, but perhaps of conducting pòwer universally. Not hearing that any chemist or electrician has attended to this busines, I have, at length, resumed the subject, though not with every advantage that I could have wished. I have, in a great measure, however, succeeded in the principal object of my inquiry; and I shall now lay before this Society, the result of my experiments and observations.

I shall begin with correcting a mistake I lay under at the time that I made the former experiments. Having been informed by persons, who attend the making of pit charcoal, that it was considerably increased in bulk after the process; I imagined that all other substances received an increase of bulk, when they were reduced to a coal; but the first experiments that I made, convinced me of my mistake. All vegetable substances are considerably contracted in all their dimensions, by the process of coaling, and the more perfect this process is, that is, as will be explained hereafter, the greater heat is applied in  
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the course of it, the greater is the diminution. I have even reduced pieces of wood to little more than one fourth of their original length and breadth, in a common fire, by the use of a pair of hand bellows only. And this was the case equally with wood of the firmest texture, as ebony; that of a middle texture, as oak; and that of the loosest, as fir, &c.

As moisture, and, I believe, small degrees of heat or cold, affects wood much more sensibly across the fibres than along them, it might have been supposed, that when wood was reduced to a coal, by the application of a greater degree of heat, the same rule would have been observed; but I found very little difference in this respect. To ascertain this circumstance, I took, from the same board, two pieces, each  $2\frac{1}{2}$  inches in length. In one of them, the fibres were divided, in the other they were not; and after coaling them thoroughly together, in the same crucible, I found that the former measured 2.05 inches, and the latter 2.15. Their conducting power could not be distinguished.

A more particular account of the degree, in which wood is shortened in coaling, will be seen afterwards, when the variations in this respect are compared with the variations in the power of conducting electricity.

To my great surprize, I found animal substances not reduced in their dimensions by the process of coaling. This, at least, was the case with some pieces of ivory, several inches in length, and a piece of bone. They bore a very intense heat for many hours, and came out of the crucible considerably diminished in weight, but hardly so much as distorted

storted in their shape, as is remarkably the case with wood, and, I believe, all vegetable substances.

In examining mineral substances, I found that my information, mentioned above, was just. Coals are very much enlarged in their dimensions by charring ; but the experiment must be made with great care, to judge of this circumstance ; for, unless the operation be very slow, the coal will retain nothing of its former shape, having been made in some measure fluid by the heat. The inside of all pieces of pit charcoal is full of cavities, and there is generally a very large one in the center of every piece ; so that, the dilatation is nothing like the extension of fibres ; but is produced by the elasticity of the new formed vapour, in forcing its way out, while the substance is soft.

With respect to the main object of my inquiry, I presently satisfied myself, that the conducting power of charcoal depends upon no other circumstance than the degree of heat, that is applied in the process of making it. I had not suspected this ; but numberless experiments clearly proved it. Taking an iron pot filled with sand, and putting into it pieces of wood, cut out of the same plank, marking them, and carefully noting their places in the pot, I always found that those pieces came out the best conductors, that had been exposed to the greatest heat. The result was the same when I made coals of bits of wood, placed one above another, in a gun barrel, one end of which was made red hot, and the rest gradually cooler and cooler.

Taking pieces of charcoal that conducted very imperfectly, or not at all, I never failed to give them the

the strongest conducting power, by repeating the process of coaling, either in a crucible, or a gun barrel, covered with sand, and kept in an intense heat.

I could not find that the mere continuance of the same degree of heat had any effect with respect to the conducting power of charcoal.

Macquer and other chemists define charcoal to be wood burned, without being suffered to flame; but, with respect to its conducting power, and, I make no doubt, with respect to all other essential properties also, it makes no difference whether it flame or not. I have coaled pieces of wood, both in gun barrels, and in crucibles, slightly covered with sand, and have let the inflammable vapour that exhaled from them take fire, at various distances from the substances; and I have also put pieces of wood in an open fire, and urged the heat applied to them, with a pair of bellows; and in all these cases have found the charcoal equally good. In the last method, indeed, very little of the substance is preserved; but the little that doth remain, after it has ceased to flame, whether it be quenched immediately, or not, conducts as well as any charcoal whatever. But one can hardly be sure that the same degree of heat is given to every part of a piece of wood, except it be exposed to it for some time; and in an open fire, urged with a pair of bellows, the wood wastes as fast as it is red hot, before the center of it is much affected with the heat.

When once any degree of conducting power is given to a piece of charcoal, I never found that it was afterwards lessened. A partial consuming of

it in an open fire doth not affect the remainder, as I observed in the account of my former experiments.

I had imagined, that the solidity of substances converted into charcoal would have had a very considerable effect on their conducting power afterwards; but the conjecture was not confirmed by experiment. Coals made of the lightest woods conducted, as far as I could perceive, as well as those that were made from the most solid, if they had been exposed to the same degree of heat in the process. Fine shavings of fir, the thin coats of an onion, the lightest sort, and every other vegetable substance that I tried, conducted equally with coals made of oak or ebony.

I had imagined, also, that the moment a piece of wood was become black with heat, it was, to all intents and purposes, a real charcoal, and, along with other properties of charcoal, would conduct electricity, more or less: but I found, by coaling several pieces very slowly, that they would not conduct in the least degree, not only when they were made superficially black, but likewise when they were black quite through, and had remained a long time in the heat that made them so; so that no eye could distinguish them from the most perfect charcoal.

I have sometimes found charcoal in such a state, that it would assist the passage of an explosion along its surface, when it would not conduct a shock any other way.

In order to satisfy myself in what proportions the diminution of weight, the decrease of bulk, and the conducting power of wood and charcoal, corresponded to

to one another. I took several pieces from the same plank, and having carefully weighed and measured them, converted them into coals very slowly, and by a gradual increase of heat, on an iron plate, held on the fire, turning them constantly, to prevent their catching fire. The following were the results.

A piece of very old dry oak, weighing 12 grains, and which conducted in the imperfect manner that wood generally does, from the moisture it contains, was, after the loss of about one grain, no conductor at all ; and it continued the same as baked wood, till it was reduced to 4 grains, when it was black quite through ; and even then, no part of it conducted, except one corner, where it had catched fire.

Another piece I carefully weighed and measured several times, in the course of the process. At first it weighed 12 grains, when its dimensions were 2 inches and .45. At 8 grains they were 2 and .4; at 5.5 gr. 1.91 and .4; at 3.5 gr. 1.8 and .35. It was now become an imperfect conductor. I then urged it with a strong heat, in a crucible, and taking it out, it weighed 1.75 gr. and measured 1.6, and .3. It was now a perfect conductor ; and though I afterwards kept it in a very intense heat several hours, by which it was reduced to 1 gr. and measured 1.6, and .3, its conducting power was not sensibly increased ; but it was become very brittle or friable.

It appears from these experiments, that these pieces of wood were reduced to about  $\frac{1}{4}$  of their weight before they would conduct at all ; though, at the same time, they were diminished in length (*i. e.* along the fibres) only  $\frac{1}{10}$ . The breadth and thickness could

not be measured with sufficient accuracy in these small pieces. To make them perfect conductors, they were reduced to about  $\frac{1}{10}$  in weight, and  $\frac{1}{2}$  in length.

A variety of circumstances led me to conclude that the cause of blackness, and of the conducting power in charcoal, is the oil of the plant, made empyreumatic, and burnt to a certain degree. I therefore conclude that these properties are some way connected with that part of the inflammable principle, otherwise called phlogiston, that is fixed and united to the earth of the plant, when the union is strengthened by an intense heat.

The sand, with which I covered the substances that I converted into coals, and also the pipe clay which I sometimes put over them, contracted a blackness like charcoal, and would often conduct pretty well. Sometimes they would conduct a shock. This must have been owing to the oil they received from the substances out of which it was expelled by the heat. In the experiment of the gun barrel fitted with pieces of wood, mentioned above, the uppermost pieces were not in the least burnt. They could hardly have been hot; yet, having contracted a superficial blackness, from the vapour of the oil expelled from the piece below them, they would even conduct a shock, though not in the most perfect manner.

Sometimes those substances that had no phlogiston themselves, but received it in consequence of being placed in the neighbourhood of other bodies out of which it was expelled, would not conduct immediately; but would be made to do so by being exposed to

to a greater heat, which more thoroughly burnt the oil with which their pores were filled.

I put a piece of common pipe into a crucible, in which I was burning some turpentine (which will be mentioned below); and it came out black quite through, like a pipe in which tobacco has been frequently smoaked. In this state it would not conduct at all; but, putting it into a crucible, covered with sand, I treated it in the same manner as I would have done a piece of wood, in order to coal it, and it came out a very good conductor. Had it been burned in the open fire, the phlogiston would have escaped, and the pipe would have been left white as at first.

Being convinced that the conducting power of charcoal depended upon the oil, or rather the phlogiston contained in the oil, and on the degree of heat with which it was burned, I took several methods to give vegetable substances more of this principle; or at least endeavoured to make them retain more of it than they usually do, in the process of coaling. But I had no apparent success in those experiments.

I began with plunging a piece of old dry oak in oil; and then, pumping the air out of it, let it stand *in vacuo* a day and night, in which time it seemed to discharge a great quantity of air; after which, I let into the receiver the air, and thereby forced the oil into its pores. But the coal from this wood was not sensibly better than others. The application of heat may perhaps expell the phlogiston in such a manner, that the residuum, being fully saturated, can retain no more than a certain proportion. I made

coals of other pieces of wood, when they were covered with cement; and I also coaled several pieces together, that they might receive phlogiston from one another; but, in both cases, without any sensible improvement in the quality of the coal.

In order to prevent the escape of the phlogiston belonging to the substance to be reduced to a coal, I put some pieces of wood into a gun barrel, and corked it as close as I could, at the same time covering the cork with cement. In this case the rarefaction of the exhaling vapour never failed to drive the cork out; but it must have been after a considerable resistance to its escape. However, I could not perceive any peculiar excellence in the charcoal made in this manner.

I do not, indeed, know any method in which differences in substances that conduct so well as these can be accurately tried, at least none that can be applied in this case. The charcoal I can make in a common fire, by the use of a pair of hand bellows, I cannot distinguish, with respect to its conducting power, from the most perfect metals, gold and silver; either by the length of the electric spark, the colour of it, or the sound of the explosion. I make no doubt but that wood, in the process of coaling, may easily have a degree of conducting power communicated to it, exceeding that of lead, iron, or the other more imperfect metals.

We may, perhaps, be guided in our conjectures on this subject, by considering the degree of heat that is necessary, either to unite the phlogiston to its base, or to separate them, both in the case of wood, and the different metals. Lead is very easily calcined, and

and it is also known to conduct electricity very imperfectly. Iron soon turns to rust ; and its conducting power I found to be very small, in comparison with that of copper, or the more perfect metals. If, therefore, in making charcoal, a degree of heat be applied greater than is necessary to calcine or revivify a metal, we may perhaps conclude, that the conducting power of the charcoal will be superior to that of the metal. As it may be possible to give charcoal, when cut off from any communication with the external air, a greater degree of heat than silver or gold would bear, without being dissipated in vapour ; it may even be possible to make charcoal that shall conduct electricity better than those most perfect metals.

Had there been any phlogiston in water, I should have concluded, that there had been no conducting power in nature, but in consequence of some union of this principle with some base. In this, metals and charcoal exactly agree. While they have the phlogiston, they conduct ; when deprived of it, they will not conduct.

I believe, however, that all vegetable, or animal substances, that contain phlogiston may be reduced to a coal ; and if the heat applied in the process be sufficient, that coal will conduct electricity. Flesh, glue, bones, and other parts of an animal body, make good conducting charcoal.

The only approach, or seeming approach, I ever made towards retaining more phlogiston than usual, in wood reduced to a coal, was by the slowness of the process. For I always found that, if the heat was applied very gradually, less volatile phlogiston,

*i. e. less*

*i.e.* less inflammable air, was expelled; and therefore I suppose that more of it was fixed. I could never afterwards, by equal degrees of heat, make this coal to weigh as little as another that was first coaled by a sudden heat.

I took two pieces of dry oak, the contiguous parts of the same stick, each weighing exactly 14 grains. One of these I heated suddenly. It yielded 8 ounce measures of inflammable air, and then weighed 2 grains. The other I heated slowly, but as vehemently, at the last, as the other. It yielded only 1  $\frac{1}{2}$  ounce measures, and weighed 3 grains.

I repeated the same experiment several times, and always with nearly the same result.

Examining the conducting power of the pieces of charcoal, made with these different circumstances in the process, I could not distinguish which were better. Perhaps a more accurate method of trying them might show, that those which were coaled slowly were the better conductors; unless, which is not improbable, the goodness of the conducting power consist in the completeness of the union that is produced between the inflammable principle and its base, which will depend upon the degree of heat only, and not on the quantity of phlogiston thus united to the earth.

N. B. To catch the inflammable air, set loose in making charcoal, I put the substances into a gun barrel, to which I luted a long glass tube, and to the tube I fastened a bladder, out of which the air was carefully pressed.

As metals and charcoal agree in consisting of phlogiston united to an earthly base, and also in conducting

ing electricity, I suspected that these two different substances might also agree in their readiness to expand by heat. Mr. Simeaton was so obliging as to assist me in my attempts to ascertain this circumstance, by the application of his excellent pyrometer. Though we could not make the experiment with all the exactness that we could have wished, yet the result of near thirty trials was uniformly in favour of the greater degree of expansion, by heat in the charcoal, than in wood of the same kind, as we imagined, out of which it was made. In general, the expansion of the charcoal was about double to that of the wood.

It is evident that a certain degree of heat makes wood and charcoal expand, and also that a greater degree of heat makes them contract. I wish we had an instrument to ascertain the precise degree of heat, at which the expansion ceases, and the contraction begins; and whether the two effects be produced by the same gradation.

In the course of these experiments on charcoal, I met with a substance, the conducting power of which is singular, and exhibits a beautiful appearance. In order to see what would remain after burning a quantity of turpentine in a glass tube, I covered it with sand, in a crucible, in the same manner in which I used to make charcoal; and, after letting it continue a sufficient time, in a very hot fire, and the flame had long ceased, I examined the tube, and found that it had been melted; but, instead of any thing like charcoal, or the least blackness, I observed that the tube was uniformly lined with a whitish glossy matter, that I could not scrape off. Upon trying

trying whether it would conduct electricity, I found it transmitted the smallest shocks, to a considerable distance; and, what appeared very remarkable, the path of the explosion was luminous all the way, and seemed to consist of a prodigious number of small separate sparks, scattered to a great distance, exhibiting such an appearance as would be made by firing gunpowder scattered carelessly in a line. The explosion very much resembled the firing of a squib. To compare it to another electric appearance, it was like the explosion passing through a thin surface of gilding.

I imagine that, though I could not perceive any interruption in this white coating, not even by the help of a microscope, it must, in fact, have been full of interstices, and the electric sparks could only be visible in passing from and conducting one particle to another.

In this experiment, I often got pieces of glass very imperfectly covered, with intervals in the white coating very large and visible; but, though I exposed the same pieces of glass to catch more of this matter, I never could get a coating of it so thick, but that, in transmitting the electrical explosion through it, it exhibited the same luminous appearance, as if there were interstices in the circuit.

I got the same matter from oil of turpentine, and oil of olives; but not from bees-wax, or spermaceti oil; perhaps not from any animal substance.

In order to observe the progress of this incrustation, I poured oil of turpentine on some flat pieces of glass, and burnt them on an iron plate, in the open fire, the heat being moderate; but the effect was a black

black covering, like soot, which would not conduct in the least. But these same pieces of glass, thus covered with the black coating, being put into a crucible full of sand, and urged with a strong heat, came out white, and conducted exactly as before.

With a less degree of heat the black covering was changed to white, but it did not adhere so firmly to the glass, as when the heat had been greater ; though it adhered more closely than the black covering, which might be wiped off with a feather. But this white coating, produced by a moderate heat, would not conduct at all.

In some cases I have found this whitish matter to be dispersed by several explosions, as Mr. Franklin found gilding with leaf gold to be.

In whatever manner the pieces of glass were covered, the coating vanished when it was made red hot in an open fire ; and the glass that remained would not conduct, any more than it did before. This circumstance exactly resembled the escape of phlogiston from charcoal and metal, burnt in the open air.

In a microscope, this whitish matter looked exactly like metal, or rather some of the semi-metals, having a bright polish, tho' it soon became, as it were, tarnished.

To try whether it was metal, I dipped the pieces of glass that were covered with it in the acids, but found that they had little or no effect upon it, though it is by no means fixed in the pores of the glass, but covers it quite superficially.

It was not in the least affected by the magnet: Upon the whole, the matter that forms this coating of the glass seems to be a kind of charcoal, only white instead of black.

Considering that metals resemble charcoal, in that they consist of an earth united to phlogiston, and that charcoal will not consume without burning in the open air (there being, probably, something in the atmosphere with which it can unite, on the principle of chemical affinities, the moment it is separated from the metallic base) I imagined that metals might not calcine or vitrify except in the same circumstances, and the event verified my conjecture.

I took a certain quantity of lead, and having put it into an open crucible, observed that it was all vitrified in 10 minutes; but the same quantity of lead, covered with pipe clay, and sand, was kept several hours in a much hotter fire, and was hardly wasted at all, the bottom of the crucible only being slightly glazed; it having been impossible wholly to exclude all access of air, and some being necessarily in contact with it when the process began. Treating charcoal in the same manner, I could never prevent some loss of weight, when the crucible was kept in a very hot fire, for several hours.

As, by this process, lead will bear a much greater degree of heat than would calcine or vitrify it in the open air, I should think it probable, that lead thus prepared must have the phlogiston more closely united to its earthy base, and be thereby a better conductor than common lead; since this is the case with charcoal thus treated. Perhaps lead, and other base

base metals, may have their quality altered, and be improved in other respects also by this proof; though they should not be changed into gold by it. The specific gravity is not changed by this process; so that, alas! it is still lead.